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## *On the Error of the Tabular Place of Venus on December* 8, 1874. By Colonel J. F. Tennant, R.E., F.R.S.

(*From a Letter to E. Dunkin, Esq., F.R.S.*)

After coming here last January I compared the tabular places which I got for the transit of *Venus* by the Astronomer Royal's tables with those I had used. I found that the printed tables were $2''\cdot03$ in excess of mine in the R.A. of the Sun. I was anxious to find the source of this before saying anything about it, but many interruptions have occurred. All I can say is, that making a third computation in the same way that Captain Campbell and I had deduced all the formulæ, I got results only numerically different in the last place from those I had got before. It is, nevertheless, a fact that the Tables published at Greenwich are right; but it is curious that Captain Campbell and I, using an arithmometer in computations, should have entirely agreed, and that a third calculation with a different instrument gave the same result, all *three being wrong.*

Now to the effects. In the *Monthly Notices* of November 1875, you gave errors of the place of *Venus*. These are affected in R.A., and I have already sent you a correction to the difference in N.P.D. With the new correction they should stand:—

Correction to $(\text{R.A.}\ ♀) - (\text{R.A.}\ \odot) = +6''\cdot50 - 0''\cdot071\ d\text{L} - 0\cdot989\ d\pi,$

„ $(\text{N.P.D.}\ ♀) - (\text{N.P.D.}\ \odot) = -2''\cdot24 - 0''\cdot017\ d\text{L} - 2\cdot689\ d\pi.$

*Calcutta,*
1876, *November* 3.

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## *On some Phenomena of the Internal Contacts common to the Transits of Venus, observed in* 1769 *and* 1874, *and some Remarks thereon.* By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

When the Observations of the Transit of *Venus*, 1874, are published in detail, I hope to examine the observations of the internal contacts with reference to their bearing on our determinations of the Sun's distance. I should have preferred not to have written upon the subject before the whole of the materials collected were available. But an impression has been slowly hardened into a conviction that none of the skilled observers, who, in 1874, observed the internal contacts with good instruments, saw the so-called black-drop phenomenon. Such an impression is, I am quite sure, unfounded, and it is perhaps desirable that this should be pointed out. The essential facts

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are in this case, as in too many other cases of the kind, becoming veiled in a mist of words.

Undoubtedly, if we are prepared to assume, on *à priori* grounds, that what a good observer, provided with a good instrument, should see, is the simple geometrical contact at a point, and that any observer who has seen more, must be either an unskilled observer, or have been provided with a defective instrument, we shall find no difficulty in establishing, to our own satisfaction, the conclusion which I have mentioned. But, on the other hand, there will be no difficulty in showing that observers have seen the "black-drop," who would have been considered observers of skill, and to have been provided with good instruments, had their observations not failed to satisfy some preconceived ideas of the nature of the phenomena which should be presented at the internal contacts. And here, perhaps, I may be permitted to add a word or two in the way of a caution. Nothing whatever depends upon the phrase "black-drop" or "black-drop phenomenon." This is merely the way in which one observer, in 1769, thought proper to describe the lingering nature of the contact, which is the cause of the only systematic error to be feared in a discussion of durations derived from internal contacts, and the cause of the principal systematic error in a general discussion of contact observations.

Whether the observer prefers to speak of the disturbance of the apparent limb of the Sun near the point of contact, as an "ombre," a "black-drop," a "ligament," a "thread," or merely to assert generally, that "the contact was gradually established," is a point of very little importance.

The important point connected with these protracted contacts is the sensible time over which the contact extends; this renders no longer admissible the assumption that all contacts take place with the same angular separation of the centres of *Venus* and the Sun. For if, at ingress for example, after the geometrical contact appeared to be established there still continued for thirty or more seconds some disturbance of the Sun's apparent limb, near the point of contact; the time at which the observer saw the geometrical contact, and the time at which he last saw any disturbance of the Sun's limb near the point of contact, cannot be combined together in any mathematical investigation without allowing for the relative change in the positions of *Venus* and the Sun in the thirty or more seconds which elapsed between the observations, or without accepting residual errors of fifteen or more seconds. And when we have to combine observations of contacts made by different observers, we cannot proceed a single step without an assumption. We have to assume that contacts of apparently the same class take place at all the different stations with the same angular separations of the centres. Unless this assumption is approximately true for some class of contacts, attempts to determine a value of the solar parallax from contact observations in a transit of *Venus* must entirely fail. It is, how-

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ever, a pure assumption, which may or may not turn out to be correct. Like other assumptions of the class, it can only be proved or disproved by logically tracing its consequences, and comparing them with the known facts, which are the observations made at the different stations.

But suppose, as will be found to be the case, that some of the observers, who have seen the contact of a lingering or protracted character, have preferred to give as the time of contact the times when they saw the so-called geometrical contact, whilst others have preferred to give the times at which at ingress they last saw, or at egress first saw, any disturbance of the Sun's apparent limb near the point of contact. How are these incongruous observations to be combined? It is clearly useless to attempt to combine them under a single assumption, that all the contacts took place at the same angular separation of the centres of the planets, unless we are prepared to admit, in the comparison of our theory with the observations, residual errors, or so-called errors of observation, at least equal to one-half the time over which the contacts have been observed to be prolonged. Enough is already known of the observations made in 1874 to justify the statement that these contact observations thus treated cannot lead to any accurate value of the solar parallax without the adoption of some method of selection of materials equivalent to the rejection of a considerable number of observations. If we were sufficiently acquainted with the causes which produce the appearances of prolonged contact, and were in possession of the requisite data upon which to base the necessary calculations, our proper course, to utilise the whole of the contacts, would be to adopt as our standard phase the *real contact*, an expression which in the case supposed would have a clear, distinct, and definite meaning, and to apply corrections to the times of the observed contacts to reduce them to the corresponding times of real contact. But an error of a tenth of a second of arc in assumed difference of semi-diameter is equivalent to an error of three seconds in time in the observed contact. I am afraid that we possess neither the theoretical knowledge nor the requisite data to render such a method available within allowable limits of error. If such be the case, we are thrown back upon the method which I adopted in my paper on the "Rediscussion of the Transit of *Venus*, 1769." Just as we assume that all the observations of geometrical or apparent contact are seen at some definite angular separation of the centres of the planets, so must we assume that all the observations of the first and last disturbance of the Sun's limb near the point of contact take place at a definite angular separation of the centres. The second assumption is as legitimate as the first; its approximate truth can only be proved or disproved in the same way, by logically tracing the consequences of the assumption and comparing them with the observations. In practice we must expect to find both of these assumptions satisfied with greater or less accuracy, according as

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greater or less uniformity has been secured in the instrumental equipment of the observers, the brightness of the field of view in which the observations were made, and in points to which the attention of observers was specially directed.

In the transit of 1769 there were ten complete observations of duration from internal contacts. The smallness of the residual errors in my rediscussion of these observations proves that, so far as they are concerned, the assumptions to which I have called attention were at least very approximately satisfied. Whether such will be the case with the contact observations of 1874 remains to be seen. The similarity of the phenomena of the contacts observed in 1769 and 1874, to which I shall call attention, may probably have some effect in reassuring astronomers of the objective character of the phenomena, and may thus render more general the acceptance, as at least *à priori* probable, which is all that is required, of the assumptions which I have already mentioned and of the other supplementary assumptions of the same class, which the want of uniformity in instrumental equipment and attempts to introduce subsidiary phases, which appears to have been made in 1874, may be found to have rendered necessary.

I have entered into these details because they must be understood before the importance of the remarks made by the observers can be appreciated, and because the principle upon which my "Rediscussion of the Transit of *Venus*, 1769," is based has been entirely misunderstood by one popular writer on the subject.

In the present paper I shall make no further reference to my own observations than to say, that I saw the so-called penumbra of Cook and Green. The light of the penumbra was, however, so faint in comparison with the direct light of the Sun that, when attention was concentrated on the contact all distinction between the central darker portions of the planet were lost sight of. The disturbance of the Sun's limb at egress appeared to me to commence some considerable time before the apparent contact, when, in fact, the limb of *Venus* was more than the second of arc from the apparent limb of the Sun. At first, however, the disturbance was of a very slight character. The appearance at first presented was something like that of a hair in vibration before the eyes. Such an appearance I consider as an indication that some of the pencils of light which had previously reached the eye from the Sun's disk were more or less interrupted by the planet *Venus* or its atmosphere. The appearance of disturbances soon assumed a definite character; and I should be content to describe the connection between the limbs of *Venus* and the Sun as a ligament. The apparent contact was rendered more difficult than would otherwise have been the case by the appearance of this ligament, or, in other words, by the disappearance of a sensible portion of the Sun's limb near the point of contact. The observations of Contact were made in a rather bright field of view.

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The definition was good, the sky perfectly free from clouds, and there was but little wind.

I am aware that it has been stated that my observations have been affected by my theoretical views; and, as the same charge might probably be brought against some other observations to which I could appeal, I shall, for my present purpose, confine myself to observations made by the French observers. The instrumental equipment of these observers was supposed, before the transit, to be sufficiently perfect to prevent any instrumental defects affecting their observations. It is clear, also, that the observers, if biassed at all, were not biassed in favour of any views of mine, and their observations have been printed in some detail, and are available for general examination. The observations to which I shall first appeal to prove that the phenomenon of the black-drop did present itself in the transit of 1874 are those made by M. Janssen at Nagasaki. M. Janssen, in describing the contact at ingress in the *Annuaire*, 1876, says that "le premier contact est obtenu par M. Tisseraud et par moi. Dans l'equatorial de 8 pouces, dont la lunette est très-bonne, l'image de *Vénus* se montre très-ronde, bien terminée, et la marche relative du disque de la planète, par rapport au disque solaire, s'exécuta géométriquement sans aucune apparence de ligament ni de goutte." Nothing can apparently be clearer than the statement of M. Janssen; and it has therefore been seized upon as conclusive evidence against the black-drop in 1874. But M. Janssen continues his account of his observations as follows:—"Mais il s'ecoula un temps assez long entre le moment où le disque de *Vénus* paraissait tangent intérieurement au disque du Soleil et celui de l'apparition du filet lumineux." This prolongation of the disturbance of the Sun's limb near the point of contact after the geometrical contact, for a "temps assez long" constitutes what other observers have called, and I should call, the black-drop. M. Janssen has apparently confined himself to giving the time at which he saw the geometrical contact; but he might, had he pleased, have given us instead the time later by the "temps assez long" which corresponded to the first appearance of the "filet lumineux." To show that this is really the point upon which controversy has hinged, I will give, for comparison with Janssen's Observations, those made by Wales and Dymond in 1769.

These observers state, "We took for the instant of last internal contact the time when the least visible thread of light appeared behind the subsequent limb of *Venus*; but before that time *Venus's* limb seemed within that of the Sun." It is clear, therefore, that Wales and Dymond in 1769 and Janssen in 1874 saw similar phenomena; but that Wales and Dymond preferred to give the time corresponding to the last appearance of any disturbance, or to the first appearance of the "filet lumineux"; whilst Janssen preferred to give the time corresponding to the geometrical contact. In the *Annuaire* for 1876—where are also

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some very important remarks by M. Janssen which appear to prove, in the most conclusive manner, the objective character of the phenomenon—M. Janssen mentions that, after the geometrical contact, he proceeded to take photographs, and that these photographs showed that the connection between the limbs of *Venus* and the Sun had not ceased.

At the egress, M. Janssen's observations would appear to have been picked up through clouds, and the details are wanting.

The evidence thus afforded by M. Janssen of the existence of the black-drop in 1874 is not only of the most conclusive character, but it is above any suspicion of being the result of preconceived ideas. It would appear that the observer had some misconception of the real character of the black-drop, and his evidence in favour of its existence is given quite unconsciously. The important fact mentioned by M. Janssen, that after his observations of the geometrical contact the photographs still showed that the connection between the limbs had not ceased, proves that the prolonged contact is not a mere optical delusion, "for photographs have no nerves," to whatever other sources of error they may be liable. It may be remembered that soon after the transit great stress was laid upon the supposed fact that Capt. Abney had secured a photograph of the contact, and that there was no appearance of any black-drop. It was, however, found, on examination, that the contact had not even commenced at the time the photograph was taken; but the impression made by the original statement, although admitted to be erroneous, has never, I fear, been removed. The contradictions of such statements do not usually receive the same publicity, or attract the same attention, as the original statements.

The next observations to which I shall refer are those made by Capt. Mouchez at the Island of St. Paul. Capt. Mouchez's telescope had an aperture of eight French inches. The following account of these observations is taken from the *Annuaire*, 1876:

Speaking of the internal contact at ingress, Capt. Mouchez says:—"Les nuages devenaient de plus en plus rares, le ciel plus transparent, les images d'une très-grande netteté. Un quart d'heure environ après le premier contact, quand la moitié de la planète était encore hors du Soleil, j'aperçus subitement tout le disque entier de *Vénus* dessiné par une pâle auréole, plus brillante dans le voisinage du Soleil qu'au sommet de la planète. Pour bien constater que je n'étais pas le jouet d'une illusion, je pris immédiatement des mesures micrométriques du diamètre de *Vénus*, dans le sens de la ligne des centres et dans le sens perpendiculaire; bien que le premier de ces diamètres fût encore à moitié hors du Soleil, et limité extérieurement seulement par l'auréole, j'obtins, à 1 seconde près, exactement la même valeur: c'était donc bien le disque entier très-net de la planète que j'aperçevais.

"Cette apparition, aussi rémarquable qu'inattendue, peut sans doute être attribuée en partie à l'atmosphère solaire rendue

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visible par contraste, en partie aussi par l'atmosphère de *Vénus*. Le ciel était devenu si pur à la suite de la tempête et l'auréole était si brillante, qu'on peut voir sur nos photographies des traces de ce curieux phénomène. Le deuxième contact fut observé dans de bonnes conditions vers $7^h 30^m$; mais, au lieu de voir l'apparence de la goutte noire, qui a pour effet de tenir les deux cornes encore séparées après le contact, j'ai vu le phénomène tout contraire: l'arc lumineux de l'auréole réunissait ces deux cornes avant le contact. Nous nous trouvions évidemment dans des conditions exceptionnellement favorables sous le rapport de la pureté de l'atmosphère et de l'excellence de notre lunette."

It is to be regretted that Capt. Mouchez has not stated more clearly whether there was or was not any disturbance of the Sun's limb after the contact.

It should be noticed that the circumstances under which the auréole first presented itself to Capt. Mouchez are exactly similar to those under which it first appeared to Wales and Dymond in 1769.

These observers state, "Soon after *Venus* was half immerged, a bright crescent or rim of light encompassed all that part of the circumference which was off the Sun, thereby rendering her whole periphery visible. This continued very bright until within a few minutes of the internal contact, and then vanished away gradually." Upon these points see the observations of M. Heraud, which will be subsequently given. The observations of Capt. Mouchez are chiefly valuable, for my present purpose, for the light which they throw upon Cook and Green's observations in 1769. The auréole is nothing more than Cook and Green's penumbra. I shall have something more to say upon this point with respect to MM. Heraud and Bonifay's observations.

The following accounts of the observations of MM. Heraud and Bonifay at Saïgon are extracted from the *Comptes Rendus* (25 Janvier 1875):—

"*Observation de M. Heraud.*

"Objectif de 160 millimètres: grossissement, 155.

"*Entrée.*—A $21^h 17^m$ la planète étant déjà entrée de plus de deux tiers, je remarque que la partie extérieure de son limbe est nettement indiquée par un filet lumineux pâle, qui, réuni aux franges de l'image intérieure, dessine un rond parfait. Ne m'attendant pas à ce phénomène, je ne puis noter l'instant précis de son apparition; l'heure ci-dessus est donnée à 1 minute près.

"L'échancrure s'arrondit de plus en plus, je suis le rapprochement régulier des pointes brillantes du croissant solaire; à un moment donné, ces pointes me paraissent immobiles. Je ne vois plus la petite auréole extérieure, la partie noire de l'échancrure me paraît absolument ronde et tangente à la ligne fictive qui fermerait le bord du Soleil. Je donne un top et, perdant de

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vue les franges, je crois un moment que le contact s'est produit et qu'il est perturbé par la goutte noire. Je regarde avec attention, et 20 secondes plus tard je note l'apparition entre l'image noire et le fond du ciel d'une lueur très-pâle teintée de noir en son milieu; cette lueur, qui arrive comme un transition entre l'obscurité et la lumière, s'agrandit et s'anime, et, en même temps, la petite tache noire devient plus petite. Je la signale comme formant une sorte de pont obscur entre le bord des astres qu'elle laisse cependant distincts; elle disparaît presque aussitôt, et le filet lumineux est dépouillé de tout trouble, les franges reprennent leur netteté autour de la planète. Les phénomènes de l'entrée sont accomplis. . . . .

"*Sortie.*—Les images sont moins calmes que dans la matinée, mais encore nettes; l'image de la planète est toujours bien frangée.

"La planète se rapprochant de plus en plus, je suis attentivement le filet lumineux. Je signale successivement l'apparition d'un filet lumineux très-faible, puis celle d'un filet lumineux presque nul. Le filet très-pâle est teinté de noir comme dans la matinée, et ressemble à ce qu'il était lors de son apparition à l'entrée; mais je n'ai pas revu le ligament plus net signalé dans la première observation. Toute apparence lumineuse disparaît; c'est *l'instant du contact*; la partie noire de l'image de *Vénus* est à une distance appréciable du bord du Soleil; peu d'instants après, cette distance me paraît nulle, et l'échancrure noire semble tangente au bord du Soleil; les cornes du croissant s'éloignent, mais sans prendre tout de suite l'acuïté qui correspond à une intersection géométrique; je les signale comme étant un peu émoussées, et ce n'est que trente-trois secondes plus tard que l'échancrure me paraît bien nette.

### "*Observation de M. Bonifay.*

"La lunette dont je me suis servi est de la maison Secrétan; elle a un objectif de 55 millimètres d'ouverture et un grossissement de 63.

"La mise au point sur les étoiles et sur les taches du Soleil s'opérait sans difficulté et donnait des images trè-nettes. C'est cette mise au point qui été adoptée pour l'observation.

"J'ai pu après l'observation, déterminer, avec la lunette méridienne prise comme collimateur, la mise au point sur le plan focal. Elle exigeait que le tube de l'oculaire fût légèrement moins enfoncé que la mise au point sur le cercle maximum d'aberration.

"L'observation a été faite dans le Jardin de l'Observatoire, avec cette lunette, montée en altazimut, sur une table massive, à 20 mètres à l'est de l'Observatoire.

"*Entrée.*—Quand la planète se projette sur le Soleil, je constate que l'image est noire, de teinte parfaitement uniforme et à contours très-nets; elle conserve ce même aspect pendant toute l'observation.

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"A $21^h$ $18^m$, temps moyen de Saïgon, le contour de *Vénus* extérieur au disque solaire s'illumine légèrement, à commencer par le bas de l'image, qui reste constamment plus visible que le haut. La circonférence planétaire paraît ainsi complétée d'une manière très-visible sur le ciel par cet arc lumineux qui semble la continuer exactement.

"Cet effet subsiste quand la planète avance; peu à peu le disque solaire, entre les bords voisins des deux astres, devient de plus en plus obscur à côté du futur point de contact. Quand le moment du contact approche, on ne distingue plus le bord du Soleil qui jusqu'alors se prolongeait nettement jusqu'au disque planétaire; les deux cornes de l'echancrure sont séparées de *Vénus* par un intervalle obscur; mais je continue à voir le bord de la planète, qui reste légèrement lumineuse. Cette circonférence lumineuse me paraît tangente au bord du Soleil, prolongé par la pensée dans l'ombre; c'est le moment qui me semble être celui du contact. La planète s'éloigne du bord du Soleil, laissant obscur l'intervalle qui les sépare; un filet lumineux vient compléter la circonférence du Soleil en réunissant les deux cornes de l'echancrure: l'ombre qui persiste entre *Vénus* et le Soleil n'est complètement dissipée que vingt-trois secondes plus tard. . . . .

"*Sortie.*—Les images sont ondulantes; néanmoins comme le matin, les contours de la planète sont bien tranchés; sa teinte est uniformément noire. Quand *Vénus* s'approche du Soleil, une ombre s'étend entre les deux astres; le bord du Soleil se rompt en deux cornes au point où doit s'effectuer le sortie, et les contours de deux astres en ce point deviennent invisibles. Je ne puis juger du moment du contact qu'en l'appréciant de mon mieux, en continuant par la pensée les parties invisibles des circonférences; de même dans une observation au sextant d'une hauteur de Soleil, si le bout de l'astre est en partie masqué par un petit nuage, il arrive qu'on cherche à obtenir le contact au juger.

"Quand la planète commence à émerger j'examine si sa circonférence devient lumineuse, comme le matin: le phénomène ne paraît pas se reproduire. L'échancrure diminue de plus en plus; peu à peu les ondulations du Soleil rendent son observation difficile, elle devient enfin invisible. . . . ."

In these observations we have all the phenomena observed in 1769 by Wales, Dymond, Hell, Chappe, Cook, and Green, reproduced with modern instruments, and in the eyes of observers who were certainly not biassed in favour of any views of mine. The fringe is here again the penumbra of Cook and Green. It may, perhaps, be remembered that Cook and Green, in 1769, observed the geometrical contacts of a penumbra as dark, or nearly as dark, as that of the planet, and also the geometrical "contacts of the dark body of *Venus*." The following remarks of M. Heraud are important as bearing on these points, and on the gradual establishment of the contact:

"L'apparition du filet lumineux a été pour ainsi dire graduelle; de plus, comme je l'ai dit dans le procès-verbal, la sépa-

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ration de l'image noire et des franges était plus nette, aussi nette au moins que la séparation des franges et l'image lumineuse du Soleil, et j'ai eu quelque peine à me défendre de considérer comme le bord de *Vénus* la limite de l'image noire, d'autant plus que ces franges n'ont reparu que très-lentement au point où le contact s'était fait: et c'est ainsi que j'avais noté d'abord à l'entrée le moment où la partie noire de l'echancrure me paraissait ronde et tangent au Soleil."

It will be seen, therefore, that Mons. Heraud had some difficulty in refraining from making an observation of the contact with the so-called "dark body of *Vénus*," like that made by Cook and Green in 1769, in addition to the geometrical contact of the outer boundary of *Vénus*. M. Heraud also remarks, "Pour M. Bonifay, l'arc lumineux a persisté jusqu'au moment du contact et peut-être jusqu'a près ce moment, tandis que pour moi il avait disparu: je dois dire que, preoccupé de l'observation du contact, j'avais un peu perdu de vue la petite auréole, et je n'ai pas noté l'instant de sa disparition." M. Heraud's remarks on this point agree with those made by Wales and Dymond; and I should be prepared to accept them as representing fairly my own observations.

*In discussions of Cook's and Green's observations before the appearance of my paper, the geometrical contacts of the penumbra as dark, or nearly as dark, as the planet, were taken as "real contacts" "the first or last appearances of any disturbance at the Sun's limb near the point of contact," or "the first or last appearance of the least visible thread of light." The contacts with the "dark body of *Venus*" were taken as the apparent contacts. These interpretations appeared to me as unsatisfactory. If the penumbra was as dark, or nearly as dark, as the planet, the observers who observed geometrical contacts without discovering any distinction between the penumbra, or fringe, and the central darker portions of the planet, must have observed also contacts with the penumbra. In this case Cook's geometrical contacts of the penumbra, instead of being real contacts, were apparent or geometrical contacts.

A reference to the drawings, of which copies are given in my papers, will show that at the time of the geometrical contacts with the "dark body of *Venus*," the disturbance of the Sun's limb extended over nearly a diameter of *Venus*, and that the true geometrical contact could not be thus intended. But the point which, as much as any other, influenced me, in the interpretation put upon these observations, was the remark made by Cook, that "the contacts were made gradually, and the time noted was when the thread of light was wholly broke by the penumbra." This showed that Cook and Green had seen the disturbances of the Sun's limb, before at egress, and, after at ingress, the penumbral contacts. It was clear, therefore, that Cook and Green's

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* This sentence is printed according to the MS., but some correction is perhaps required.—[Ed.]

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contacts could not be real contacts in the same sense as those made by Wales and Dymond were. The confirmation of my views, on the interpretation to be put on Cook's and Green's observations, afforded by the observations of the French observers in 1874, is remarkable.

Since the appearance of my paper I am not aware that any astronomer has seriously maintained the correctness of the interpretations which had previously been held with respect to the nature of the contacts observed by Cook and Green. One astronomer doubted the goodness of the observations, and was inclined to attribute the appearance of the penumbra to instrumental defects. The observations of the French observers appear to show that there is no longer any ground for such a suspicion, and that the observations of 1769 are as good, or very nearly indeed as good, as the corresponding observations of 1874. Any one who will take the trouble to study my papers in the *Monthly Notices* for 1868, October, will see that the interpretation which I put upon the observations of Cook and Green is the most important point in my work. In fact, the only other step of any importance was the rectification of a similar mistake in Hell's observations, indicated by the fact that different interpretations had been assigned to the word "contactus," at the ingress and egress.

Small and unimportant changes are easily enough made in all such work. But when any astronomer will prove that one or more of the twenty internal contacts included in the ten complete durations observed in 1769 has been erroneously interpreted by me, and will show that when my erroneous interpretation is removed and the correct interpretation carried into the equations of condition, the ten durations are fairly represented, and that the value of the solar parallax deduced from the equations is no longer my value $8''\cdot91$, or one differing from it by $\pm 0''\cdot07$, but is Encke's value $8''\cdot58$, or one differing from it by $\pm 0''\cdot07$—then I shall be quite prepared to admit that my work has been without value. Before this is done I can only meet general statements that the work is not accepted, by asking those who assert that such is the case to go one step further and prove that it ought not to be accepted. If the work is inaccurate, the proof should be easy; but no proof of the kind has yet been given.

*Royal Observatory, Cape of Good Hope,*
1876, *November* 6.